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THE TELEGRAPHIC JOURNAL

AND

ELECTRICAL REVIEW.

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Vol. V.—No. 111.

NEWSPAPER SCIENCE.

It must be patent to every general reader that the phraseology, and, to a certain extent, the ideas of science, are finding their way more and more into the "light troops" of literature. In these days, when popular treatises on science are said to share the attractions of Mudie's with works of travel and fiction, an affectation of scientific knowledge has become a point of fashion. Hardly a century ago the secrets of nature were regarded with a reverence amounting almost to awe. But we have changed all that; and it is as essential for a man of fashion to be acquainted with the atomic theory as that he should have his coat made by Poole. No one laying any claims to culture can in the present be without an opinion on the nebular hypothesis, the origin of species, spontaneous generation, and the telephone. The jargon of the laboratory runs through our magazines, and even sullies pure literature—a fact which is to be deplored. Homely and natural expressions are set aside in favour of terms from the text-book; and, by a process which is twice cursed, scientific nomenclature is thereby degraded into a kind of slang. We do not know whether to attribute this masquerading in the dress of science to a pretence of originality on the part of our popular writers, or to an affectation of knowledge; but in either case it is sufficiently ridiculous. Here is a slight specimen of the shoddy magniloquence with which these writers sometimes clothe their ideas: "There seems to be a degree reached in the brain barometer at which faculty undergoes a chemical change, and slips through the fingers of the educational manipulator in an imponderable ether."[*] We do not, of course, profess to understand the meaning of this passage—the lay-figure that supports such incomprehensible verbiage—but we would fain ask if it could not have been expressed without the joint assistance of a chemical laboratory and the luminiferous ether. We hear a great deal now-a-days about the "effervescence" of nations, and the "excitement of brain-cells." Penny-a-liners speak of the "ozone" of seaside air, and even the *Vers de Société* rhymer apostrophises Love, as some divine feat of molecular force." It is not, however, with these elegant smatterers that we have particularly to do in this article, but rather with the more important phase of newspaper science.

With the wide public interest in scientific matters which undoubtedly exists, and is spreading, it is of course necessary that the newspapers should report upon them; and for intelligible reporting it is necessary, to a certain extent, to explain. The attempts made in this direction by the newspapers are at least praiseworthy—as attempts; but their notorious absurdity is the laughing-stock of all who know even the elements of true science. Newspaper editors cannot too soon understand that, above all things, science is not a thing to be tampered with, and that nothing short of a special training can fit a man to write about it. It will certainly not do to ask the sporting critic to expound the Darwinian theory, or the versatile individual who pens occasional leaders on even such widely-sundered themes as the atrocities in Bulgaria, and the Dunmow Flitch, to undertake an article on the duplex system. He is able, it is true, by a fine instinct to seize on the most sensational points; but his lamentable ignorance of the subject is sure to betray him into some confusing omission or glaring error, and his natural love for the picturesque to lure him into extravagant conjectures. We repeat, science is not a thing for the uninitiated to meddle with—it is like an edged tool. The mind is conscious of a delightful facility in dealing with such questions as French politics or Russian diplomacy, but such guess-work will not do in positive science, where the fundamental requisites are "accuracy and definiteness," not only in ideas but in the use of terms. That there is a gross misuse of scientific terms in the newspapers is apparent to all scientific readers. Professor Tait, in his famous lecture on "Force," states that he had just read in one newspaper that "the force of a projectile from the 81-ton gun has at last reached the extraordinary amount of 1450 feet." We were amused the other day to find, in the preface to a celebrated and recent novel, the remark, "What are the most brilliant of our chemical discoveries compared with the invention of fire and the metals?" After this we may expect to hear of the discovery of printing and the invention of the fixed stars.

We have neither the space nor the intention to cite many samples of newspaper science, although we think that an album of the mistakes made in telegraphy alone, would make a jest-book for the electrician rivalling Joe Millar. How amused (if he was not indeed, shocked) must Sir William Thomson have been on the morning after the famous Pender Soirée to read in an enlightened and influential organ that in his Siphon Recorder, exhibited then for the first time, the current coming in from the line, passed down the siphon, and dragged the ink with it on to the paper. Of late the inventions of the duplex system, the electric candle, and the telephone have called into existence a rich crop of the finest newspaper science. A notable peculiarity in it, is the authoritative and masterly tone of these discourses. Witness, for instance, the magnificent bosh of the *Times* in a recent article on "duplex telegraphy": "The difficulties in the way of accomplishing this object were found to be much greater on long than on short lines, and very much greater on sub-marine cables than upon land-lines of whatever length. The latter differ from the former *by being more completely insulated;* the submarine cable being surrounded by water, which is in contact with the earth, and which brings them into direct relation with it. One effect of this relation is that the passage of the battery current along the wire produces an induced current around it, which delays the speed of the battery current, so that this, instead of travelling rapidly as it does upon land, travels very slowly, and flows out of the wire *in a way which has often been compared to the escape of a viscid liquid.*" The italics are ours.

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[*] *Cornhill Magazine.*

Bad enough as newspaper science is when it confines its worthy aims to enlightenment, it is doubly obnoxious when it affects to criticise. Some of our readers may remember a very flagrant specimen of this kind in the *Daily Telegraph* some years ago. The jealous author fell foul of the unoffending Joule's Mechanical Equivalent of Heat, which, if we recollect rightly, he seemed to consider a new species of currency calculated to depreciate the value of silver. In a leading article the other day on Mr. Preece's Plymouth Lecture, and the articulating telephone, the *Standard,* after supposing that "there were among his hearers—as, indeed, there may be found among classes pretending to much higher education—many who fancy that in some mysterious manner words and sentences are really transmitted along the wires," goes on to inform them that "as practically and generally employed the electric current telegraphs a message precisely as the old wooden signal posts, with moveable arms used to transmit the messages of a former generation; that is by causing signals made at one end of the line to be repeated at the other." If this explanation of the electric telegraph is not incorrect and misleading, language has no definite meaning.

The writer rises to higher flights. Here is his elucidation of Professor Bell's telephone: "A metallic disc, thin enough to vibrate quickly with every vibration of the air, and so placed that its vibrations shall be perfectly free, and shall be communicated to a sort of pencil or pointed wire, receives the sound of the human voice or that of a musical note, just as does the human ear. As the ear communicates the sound to the brain, so does this disc communicate it, by the wire, which answers the function of a nerve, to another disc at the other end. Obviously each vibration in one direction can be made to bring the point or pencil into contact with the wire, and thus establish communication and send the current, while each vibration in the opposite direction will break the contact and interrupt the current. This series of interruptions and renewals communicates the vibrations that constitute sound to the corresponding disc, just as the ordinary telegraphic signals are communicated." After evolving this ingenious conception from his inner consciousness, the writer is fairly puzzled by his own creation. He is like a spider caught in the mazes of its own web, or the unhappy poet who stood in awe of the demon of his own imagining. He is told that the confused applause of the audience was transmitted by his telephone, and marvellous as that instrument undoubtedly is, he cannot see how it is capable of transmitting such mingled sounds. He therefore assumes the authoritative air of the savant, and throws out the following valuable suggestion for the benefit of Professor Bell and his brother inventors: "If the account we have received be nowise exaggerated it would be worth while to repeat some of these experiments with special care and under changed conditions; and particularly to test very strictly the manner and accuracy of reproduction of mingled sounds from many voices speaking at once."

After the utter ignorance of this citation, glazed as it is by a veneer of superior wisdom, we may well despair of newspaper science. So long as it confines itself to such attempts at the mere enlightenment of the public, if it cannot be said to do any good, at least it can do little harm. If the quasi-scientific gibberish of physical rhetoric which ephemeral writers employ, clouds their ideas from ordinary comprehension, it is of no consequence. Their ideas may be well spared. But it is a very different affair when such periodicals assume an authority they do not possess, and deceive the public with their own extravagant conjectures and ignorant speculations. The recent sudden fall in gas-shares was mainly, if not entirely, due to the sensational articles in the newspapers on the exhibition of Jablochkoff's electric candle. As long as such important subjects are treated by writers rendered equally incapable by their want of knowledge and of prudence, the results will be ridiculous, if not decidedly pernicious.

MINING AND THE TELEPHONE.

AMONG the many uses to which it is believed the telephone may be advantageously applied is that of communication between the surface and deep workings in mines; and to test its suitability in this direction, experiments were made on Tuesday at West Wheal Eliza mine, near St. Austell. These are the first underground trials of the kind in England—and, it is believed, in the world—and they fully realised the expectations of the most sanguine believers in Professor Bell's wonderful invention. The experiments resulted from the labours of Mr. Arthur Le Neve Foster, of the India-rubber, Gutta-percha, and Telegraph Works Company (Limited), London, brother of Dr. C. Le Neve Foster, Government-Inspector of Mines for the West of England. Mr. Foster, having read in the newspapers elaborate accounts of Professor Bell's telephone, proceeded, on the information thus afforded him, to make an instrument on the principles laid down. Mr. Foster being in Cornwall just after the great sensation caused by the experiments at Plymouth, it was suggested that the telephone's value to miners, as the means of communicating from the lowest level to the surface, should be tested; the introduction of a perfect machine of the kind being of great importance. Already in large coal mines either the electric telegraph or speaking tubes have been laid, but the former is attended with some difficulty in learning, and the latter is no use at a great distance, the friction making the sounds indistinct; while both would be condemned on the score of cost. The telephone, however, presents no such disadvantages. All that has to be conveyed to any part of the mine is an insulated wire, cased in india-rubber, through which the sound of the voice can be conveyed, for whilst the speaking-tube conveys vibrations of air direct, by means of the telephone vibrations of air are altered into vibrations of electricity, which again become vibrations of air on reaching the other end of the instrument. By these means the sounds produced are much more distinct, and can be carried a far greater distance than by the speaking-tube.

The experiments were made down the main shaft of the mine. The wire was taken down in the roughest possible way by Mr. A. Foster, who went down in the gig. It was fastened in the 42-fathom level to the air pipes, and the whole time occupied in making the arrangements and commencing conversation was fifteen minutes. The results were more than could be expected. Every word from below was plain and clear, songs, and even whispers, were distinctly heard, and on being questioned by Dr. Foster from above, those below replied that they could hear the whistle of an engine on the line. The wire was cut in the mine, and two persons joined hands in keeping up the connection between the main piece and the end. These gentlemen constituted the conducting medium for the electricity, and the conversations were as plain after the severance as before. It was agreed among all the practical men present that nothing was wanting to make a telephone a great success in mining operations, seeing that an electric-bell can be attached to the wire which conveys the sound, and be worked without additional trouble. Thus the attention of men at the surface could be instantly secured, and communications carried on with the greatest speed and accuracy from the bottom of our deepest mines.

THE SPECIFIC INDUCTIVE CAPACITY OF GASES.

By JOHN PERRY and W. E. AYRTON,

Professors in the Imperial College of Engineering, Tokio, Japan.

IT was formerly supposed that because a gas offered less and less resistance to the passage of an electric spark as its density was diminished, therefore a very rare gas would offer an extremely small resistance. The invention of the Sprengel Pump enabled the fallacy of this conclusion to be apparently proved, and it is now well known that no visible spark can be sent between two platinum points very near together in a perfect Sprengel vacuum even by means of a powerful Ruhmkorff coil.

As regards the specific inductive capacity of gases the only series of experiments with which we are acquainted is that of Faraday. The means in his power, however, of obtaining a perfect vacuum, and of measuring induction, were far inferior to those which now exist, and we must not conclude that the result obtained by him, "the specific inductive capacity of all gases at all pressures is constant," either expresses an exact physical law or forms any criterion of what is to be expected in a perfect Sprengel vacuum. The theoretical conclusion, therefore, deduced in our paper on "The Viscosity of Dielectrics," that since a perfect Sprengel vacuum has the greatest known resistance it may be found to have the smallest specific inductive capacity, it is very desirable to test carefully, although this conclusion, at first sight, appears to be in direct opposition to the results obtained by so careful an experimental philosopher as Faraday.

That no visible spark can be sent by a powerful Ruhmkorff coil between two platinum points very near together in a Sprengel vacuum might be due to the vacuous space offering *not* an extremely *great* resistance, but instead only a very *small* resistance; for it is well known that no optical or heating effect is produced by an induction coil unless there is a decided break in the continuity of the secondary circuit. To examine this a hermetically sealed Alvergniat's tube was selected, in which the platinum points were about half a millimetre apart, and between which no spark was producible by a Ruhmkorff coil, which through ordinary air gave sparks 8 centimetres long. The outside of the glass was chemically cleaned, and the insulation of the tube tested by means of an electrometer. The result showed that the insulation of the vacuous space was very great; hence it is the extremely great and not the extremely small resistance of the vacuous space which prevents the production of a spark.

Working with the electrometer and such a tube, it was difficult to determine whether certain results we obtained were due to induction through the mass of the glass or through the vacuum; we had, therefore, to devise an apparatus with which this could be determined with certainty.

Although devised for experiments on the Sprengel vacuum, we have only up to the present time used it to measure the specific inductive capacities of gases at different pressures. The apparatus about to be described has, during our investigation, undergone a variety of modifications; we shall, however, describe it as it is now, and indicate some of the changes experience has taught us to make. It consists of a brass box, $22\frac{1}{2} \times 19\frac{1}{2} \times 6$ centimetres, composed of brass plates $3\frac{1}{2}$ millimetres thick, and strengthened with ribs. Eleven brass plates, 18 centimetres square, are fixed, and kept at 3 millimetres apart, by means of ebonite racks, fitted into the two ends of the box. At the bottom of the box the plates rest on a strip of ebonite, which, as well as the racks, was chemically cleaned and paraffined before being fitted into the box. The plates 1, 3, 5, 7, 9, 11 are all connected with each other and with the brass box by soldered copper wires; 2, 4, 6, 8, 10 are connected with each other and with a long platinum wire by soldered copper wires. These two series of plates form our condenser. The platinum wire passes through a glass tube $35\frac{1}{2}$ centimetres long, chemically cleaned and paraffined, and very carefully cemented into a brass tube, which forms a part of the top of the brass box. This glass tube is hermetically sealed at the end furthest from the box, and it is at this point only that the platinum wire touches the tube. An iron tube soldered quite tight into the brass box, and open at its lower end, forms a connection with the Sprengel or other air pump. This iron tube was so bent as to prevent any irregularity in the working of the Sprengel pump sending the mercury into the condenser, an accident which occurred in the early part of the experiments, when the iron tube had a different shape, and which necessitated the taking to pieces of the brass box to remove the mercury.

The three conditions to be fulfilled in the construction of this apparatus were:

1. The plates 2, 4, 6, 8, 10 should be perfectly insulated from the plates 1, 3, 5, 7, 9, 11, and from the box.
2. The condenser should be quite air tight.
3. The relative positions of the plates should not be in the least altered by any yielding of the box, &c., on exhausting the air.

Simple as it might at first sight appear to be to satisfy these three conditions, it has nevertheless taken us several months to fulfil them all simultaneously. Sometimes (possibly from want of care on the part of the workmen) the final soldering up of the box spoilt the insulation of the ebonite inside, at other times the insulation would be good, but a small air leakage would be produced by the slight distortion of the box produced on exhausting the air; various contrivances tried for leading out the platinum wire by means of a tightly fitting ebonite collar quite failed; after many trials, conditions 1 and 2 were satisfied, when it was found that condition 3 was not, for in order to get a condenser with as large a capacity as possible it was the set of plates 1, 3, 5, 7, 9, 11 that were originally the insulated set and connected with the platinum wire, while 2, 4, 6, 8, 10 were connected with the brass box, the box itself thus forming a part of the condenser; but it was found that after many trials the sides of the box approached the plates 1 and 11 a very little on exhaustion, thus increasing the capacity of the arrangement. However, by connecting 1 and 11 with the brass box, as described above, and making 2, 4, 6, 8, 10 the insulated series, this difficulty was removed.

Passing over one or two unfortunate fractures of the long glass tube, we ultimately succeeded in getting the arrangement quite satisfactory, at any rate for the best vacuums that could be obtained with a large continuous-acting Bianchi's air-pump (and we have not gone beyond this at present). The insulation too is also so good that there is no appreciable loss of charge in about one minute, when the difference of potentials between the coatings is two or three volts.

The mode of accurately measuring the capacity of our condenser when filled with air or other gases at different pressures had next to be considered. The first method we employed was as follows:—

A current from a battery of some 200 Daniell's cells joined in series was sent constantly through a large resistance. One coating of our condenser was connected with one coating of a $\frac{1}{3}$ Micro-Farad condenser, made by Messrs. Warden (of paraffined mica it is believed), and with one point of the resistance coils. Two wires attached to the two ends of the coil were used to charge the other coatings of these condensers respectively. The point of attachment of the wire to the coil was moved until it was found that when the two condensers were discharged into one another after the removal of U and V there was no remanent charge. Then if X be the capacity of our condenser

$$X = \frac{1}{3} \times \frac{\text{resistance of V S}}{\text{resistance of U S}} \text{ micro-farads.}$$

The remanent charge was measured by discharging the condensers through a Thomson's very delicate reflecting galvanometer, adjusted so that one volt through a resistance of 120 megohms gave 130 divisions on the scale at one metre distance. As it was found very difficult to adjust so that there was absolutely no remanent charge, we adjusted until the deflection on discharging was small, then keeping the ratio of the two halves of the set of coils constant, m say, we made 15 observations of the remanent charge, obtaining a mean reading of, say, a scale divisions. Now, with another ratio of the halves of the set of coils, say n, we made 15 more observations, obtaining a mean reading of, say, b scale divisions. Then since a and b are small

$$X = \frac{1}{3} \times \frac{a\,n + b\,m}{a + b} \text{ micro-farads.}$$

The disadvantage of this method was, that since the capacity of our condenser was very small compared with one third of a micro-farad, the difference of potentials between V and S was very small even when 200 cells were employed. To overcome this objection we adopted an improved, and somewhat novel, method of testing. The slide wire was attached nearer the centre of the coil, the $\frac{1}{3}$ micro-farad condenser charged with the wire connected to one end of the coils and insulated, the air-condenser was charged with the wire connected to the other end, insulated and discharged into the $\frac{1}{3}$ micro-farad condenser, separated and again charged with the first wire, and again discharged into the $\frac{1}{3}$ micro-farad condenser (which of course had not again been charged with the second wire), the air condenser was a third time charged with the first wire and again discharged into the $\frac{1}{3}$ micro-farad condenser, etc. By adjusting the slider properly and by charging the air-condenser say ten times, and ten times discharging it into the $\frac{1}{3}$ micro-farad condenser, the remanent discharge could be made nought. Or rather, having adjusted until the remanent charge after such an operation was very small, we were able, by taking the means of a number of observations to find what adjustment of S would have made the remanent charge exactly nought by a calculation similar to that given above. This being done, it may easily be shown that the value of X may be found by eliminating nine unknown quantities from ten equations. A disadvantage of this method is that some very small portion of the charge of $\frac{1}{3}$ micro-farad condenser may be lost on account of not perfect insulation, since some short time must elapse between the first and the tenth discharge of the air-condenser into it. A more serious and unexpected difficulty, however, was found to arise from an extremely slight absorption of charge that took place in the $\frac{1}{3}$ micro-farad condenser, and after many experiments we were compelled to abandon this method also, as unlikely to furnish accurate measurements of the extremely small difference in the specific inductive capacity of gases, the existence of which Faraday had not been even able to detect. From the experiments, however, made in the manner above described, it appeared that a vacuum had a less specific inductive capacity than air, but the method was not sufficiently exact to determine the exact amount of this difference.

It was, therefore, necessary to use for our standard condenser an air condenser of which the capacity would remain quite constant. This was formed of a stiff brass plate 3 millimetres thick, 42·6 centimetres wide, and 42·6 centimetres long, strengthened at the back with brass ribs; this was supported on three clean and paraffined ebonite levelling screws, pointed at the bottom, and resting on a very firm stone pillar. A piece of hard wood, coated on its upper surface with tin foil, formed the earth coating of this standard condenser. The whole was covered in with a metallic box. By raising or lowering the levelling screws the capacity could be diminished or increased. The whole, therefore, formed an air condenser of adjustable capacity, but which may be expected to keep a perfectly constant capacity as long as the levelling screws are untouched. The wooden tin foil covered plate was used in preference to a stiff brass plate from motives of economy. Our measurements, however, became so exact that the yieldings of the tin foil, although very slight, could be detected: in future investigations, therefore, we intend to substitute a stiff brass plate. We shall call the condenser just described the open-air condenser, or simply "the open," to distinguish it from the closed air-tight condenser previously described, and which we shall call "the closed."

The method of comparing their capacities was as follows:—The uninsulated coatings of the two condensers were connected with the outside of a very delicate quadrant electrometer, with one pair of quadrants, and with the earth. A battery of 87 Daniell's cells arranged in series was employed to send a constant current through a resistance coil of 10,000 ohms, the two ends of which were connected with a reversing key, by means of which either of these ends could be connected with the earth or with the insulated coatings of either one of the condensers. The object of the reversing key was to enable us to charge the condensers successively with the *whole* battery, the one receiving a positive charge in its insulated coating, and the other a negative charge. The condensers then being connected with one another their remanent charge was measured by measuring its potential with the electrometer. The use of keeping the battery constantly connected with the resistance coil was so that its electromotive force could be measured in scale divisions, by observing the deflection corresponding with the one-hundredth part.

Full precautions were taken to ensure, first good insulation of all parts of the keys employed, secondly protection from induction by our bodies, &c., on the leading wires, metal portions of the keys, &c. Between every pair of observations both condensers were always short circuited for 30 seconds. The interval between two successive series of observations was about 3 minutes, but as the substances experimented on, dry air, vacuum, &c., were taken in turn, the interval between two sets of experiments on the same substance may have been considerable. The specific inductive capacity of dry air having always been regarded as unity, we took this as our standard substance. A set of experiments on dry air was, therefore, made *immediately before and after* a series of experiments on any other substance.

In the following tables the deflections given are each the mean of ten observations, with the exception of the first number, which is the mean of eight observations. A positive deflection means the closed has a greater capacity than the open, or

$$\frac{c}{o} > 1,$$

and a negative deflection

$$\frac{c}{o} < 1.$$

The ratio of *C* to *O*, given in column 3 of the following table is calculated as follows:

Let *d* be the deflection of the electrometer corresponding with a difference of potentials equal to one-hundredth of the electromotive force of the battery, let *p* be the mean deflection given in column 2 for any of the substances, dry air, vacuum, &c., then

$$\frac{c}{o} = \frac{100\,d + p}{100\,d - p}.$$

All the gases used in the experiments were very carefully dried before introduction into the condenser.

Dielectric.	Potential of Remanent Charge. Mean of 10 Observations	$\frac{C}{O}$	Remarks.
Air	—19·5	0·9981	Pressure varying from 15 to 90 millimetres.
Vacuum........	—29·97	0·9970	
Air	—23·13	0·9977	Pressure varying from 15½ to 76 millimetres.
Vacuum........	—35·14	0·9965	
Air	—22·00	0·9978	Pressure varying from 20 to 75 millimetres.
Vacuum........	—38·74	0·9962	

The change of pressure produced in the experiments with the vacuum was due to a slight leakage in the connecting tubes, barometer gauge, &c.

One one-hundredth of the battery gave a deflection of the electrometer needle equal to 202·1 scale divisions.

To give an idea of the accuracy of the method we give as a specimen in the following table the ten observations in full, of which 38·74 recorded above is the mean.

Zero.		Vacuum Reading.		Deflection.
74·5	....	33·9	....	−40·6
74·1	....	28·8	....	−45·3
74·1	....	19	....	−55·1
74·1	....	39	....	−35·0
74·1	....	41	....	−33·1
74·1	....	41·5	....	−32·6
74·6	....	45·5	....	−29·1
75·5	....	34·5	....	−41·0
76·0	....	43	....	−33·0
76·6	....	34	....	−42·6
			Mean	−38·74

$$\frac{c}{o} = \frac{20210 - 38{\cdot}74}{20210 + 38{\cdot}74}$$
$$= 0{\cdot}99617.$$

The probable error of one of these observations, as calculated by the method of least squares, is 5·19. The probable error of the result (38·74) is 1·6, or about 4 per cent. Similarly the mean error of each set of ten observations may be taken as about 4 per cent. Now as the battery did not alter appreciably during one day's experiments, we have merely got to consider how much the ratio of C to O will be affected by an error of 4 per cent. in the mean of one set of ten observations. Using 40·3, instead of 38·74, we find

$$\frac{c}{o} = 0{\cdot}99602$$

instead of 0·99617 obtained by using 38·74, showing a probable error of only 0·00015 in the ratio.

OBSERVATIONS CONTINUED.

March 4th, 1877.

The open condenser re-adjusted:

Dielectric.	Potential of Remanent charge. Mean of 10 Observations.	$\frac{C}{O}$	Remarks.
Air	+ 43·36	1·0043	
Vacuum	+ 21·53	1·0021	Pressure about 20 millimetres.

One one-hundredth of the battery gave a deflection 201·0.

March 5th, 1877.

The open condenser slightly readjusted.

Air	+13·88	1·0013	
Vacuum	− 6·05	0·9994	Pressure 29 millimetres, slowly increasing.
Air	+20·70	1·0019	
Vacuum	+ 7·03	1·0006	Pressure varying from 24 to 29 millimetres.
Carbonic dioxide	+23·90	1·0022	Diluted with a little air.
Carbonic dioxide	+17·60	1·0016	Diluted with a little air.
Air	+11·52	1·0011	
Air	+25·79	1·0025	
Carbonic dioxide	+33·15	1·0032	Diluted with a little air.
Carbonic dioxide	+39·90	1·0038	Diluted with a little air.
Air	+39·00	1·0037	

As the exact amount of air in the carbonic dioxide of the previous experiments was unknown, we repeated the experiments with purer gas.

Air	+19·18	1·0018
Carbonic dioxide	+27·01	1·0026
Carbonic dioxide	+13·64	1·0013
Air	+ 4·35	1·0004

March 7th, 1877.

The open condenser readjusted. Slight leakage in india-rubber connecting tubes partially remedied.

One one-hundredth of the electromotive force of the battery gave 209·75.

Dry air	+ 4·50	1·0005	
Hydrogen............	+ 9·41	1·0009	Diluted with air.
Hydrogen............	+25·17	1·0024	Diluted with air.
Air	+22·98	1·0022	
Coal gas	+22·86	1·0028	
Coal gas	+36·9	1·0035	
Air	+34·23	1·0032	

One-hundredth of the electromotive force of the battery gave 206, and after some time 210.

Air	+ 14·76	1·0014	
Vacuum	+ 16·08	1·0015	Pressure varying from 5 to 74 millimetres.
Air	+ 40·67	1·0039	
Hydrogen............	+ 41·96	1·0040	Moderately pure.
Air	+ 52·04	1·0050	
Sulphuric dioxide	+75 13	1·0072	
Air	+ 47·89	1 0046	

March 11th, 1877.

One one-hundredth of the electromotive force of the battery gave a deflection on the electrometer scale of 210·5.

The open condenser slightly readjusted.

Dielectric.	Potential of Remanent charge. Mean of 10 Observations.	$\frac{C}{O}$	Remarks.
Air	−22 64	0·9978	
Hydrogen	−29 65	0·9972	Moderately pure.
Air	−29·01	0·9973	
Vacuum	−56·17	0·9947	Pressure about 5 millimetres, increasing.
Air	− 53·93	0·9949	
Hydrogen	−46·20	0·9956	Moderately pure.
Air	−39·99	0·9962	

Waited about ten minutes.

Hydrogen	−46·82	0·9955
Air	−49·12	0·9954
Sulphuric dioxide	+ 10 15	1·0009
Air	−37·83	0·9964

One one-hundredth of the electromotive force of the battery gave a deflection on the electrometer scale of 205.

By dividing the value of $\frac{c}{o}$ for any one of the substances by its corresponding value for dry air, we obtain the specific inductive capacity of the substance, that of air being called unity. As in all cases (with a few exceptions quite at the beginning), a set of experiments for dry air was made at about equal intervals of time before and after the set of experiments corresponding with any other substance, the division we have employed in each case in obtaining the following table was the arithmetic mean of the two values $\frac{c}{o}$ obtained with dry air. This eliminates the error that would otherwise be produced by the slow alteration that occurs during each set of experiments in the value of $\frac{c}{o}$ obtained for dry air.

In the last column in the following table we have given approximately the weight which, in our opinion, ought to be given to each result.

Vacuum.	Carbonic Dioxide.	Hydrogen.	Coal-gas.	Sulphuric Dioxide.	Weight of Result.
0·9984					3
0·9980					1
0·9985					2
	1·0003				0
	1·0004				0
	1·0008				2
		1·0003			0
			1·0004		2
0·9988					2
		0·9997			2
				1·0024	2
		0·9996			2
0·9986					2
		1·0000			2
		0·9998			2
				1·0050	2

The close agreement of the results for the vacuum, and of those for hydrogen, show that when the nature of the dielectric is nearly the same in all the trials, the probable error is very small. Thus the probable error of the mean 0·9998 for hydrogen is 0·00005, as calculated (by the method of least squares) from the four observations on which dependence can be placed, the first result for hydrogen being useless, as we knew that the hydrogen was diluted with air. The probable error of *each* of the above four observations, as calculated from them alone, is 0·0001, and this is not very different from 0·00015 previously found for the probable error of ten single observations for a vacuum. It would seem, therefore, that the errors are not so much due to differences in the purity of the hydrogen employed as to other unavoidable errors in the apparatus.

Taking the proper means, by using the weights given in the last column of the previous table, we find:

Substance.	Specific Inductive Capacity.	Remarks.
Air	1·0000	Taken as standard.
Vacuum	0·9985	The vacuum was always less in specific inductive capacity than air, even single observations showing a less capacity.
Carbonic Dioxide	1·0008	When air was allowed to mix with the carbonic dioxide the specific inductive capacity more and more approached 1·0000.
Hydrogen	0·9998	
Coal Gas	1·0004	
Sulphuric Dioxide	1·0037	

Thus there seems to be a connection between condensability, diffusivity, viscosity, high index of refraction, and specific inductive capacity of a gas.

The method of measuring the specific inductive capacity which we have employed is susceptible of any amount of accuracy, depending on the battery power used. If we can succeed in the plan we have been endeavouring to arrange, of using an electrical machine with our delicate electrometer, we shall be able to measure the specific inductive capacity with still greater accuracy. For a vacuum, in consequence of the facility with which electricity of high potential escapes an electrical machine will probably give less accurate results than a battery.—*Read at an Extraordinary Meeting of the Asiatic Society of Japan, April* 18, 1877.

UNDERGROUND TELEGRAPH LINES IN GERMANY.

By EDOUARD RAU.

ON the 23rd of July last there was submerged in the Rhine, near Mayence, the cable completing the one of the two great subterranean lines—that of Berlin-Mayence (Metz)—to establish which the German Parliament voted a sum last year.

Germany is, then, the first power which, after long hesitation it is true, established subterranean lines on a large scale.

The amalgamation of the postal and the telegraph administrations has powerfully contributed to accelerate this important reform. The unsuccessful attempts which took place some eleven years ago under the special direction of M. Chauvin, the then director of telegraphs for the Prussian Government at Cologne, had greatly tended to confirm the doubt of the practicability of underground telegraphs. Later on financial difficulties prevented further experiments. This difficulty now ceases to exist, in consequence of the union of the postal and telegraph services, a union which allowed of the reduction from 2,000 to 50 of the number of independent telegraph offices, and which, in consequence, reduced considerably the working expenses.

The excessive increase and continuation of the number of faults which occurred on the aerial wires led to the reopening of the question of underground wires. The following official returns will give an idea of the number of faults which have occurred. In 1874 there were 350 poles broken, 875 breaks in the wires, 2,826 interruptions from contact or earth. In 1875 there were 506 poles broken, 1,405 breaks in the wires, 4,076 interruptions from earth or contact. In 1876 the numbers were: breaks in poles, 1,708; breaks in the wires, 3,284; and 4,415 faults from earth or contact.

This increase in the number of accidents was out of all proportion to the number of lines. In 1874 the length of line in use was 31,035 kilometres, representing a mileage of wire equal to 107,645 kilometres. In 1875 there was 34,141 kilometres of line and 123,455 kilometres of wire; and in 1876, 37,264 kilometres of line and 138,891 kilometres of wire.

There were, then, in 1874, for 10 kilometres, 0·11 pole-breaks, 0·08 wire-breaks, and 0·26 earths or contacts; in 1875, 0·15 pole-breaks, 0·12 wire-breaks, and 0·33 earths or contacts; and in 1876, 0·46 pole-breaks, 0·24 wire-breaks, and 0·32 earths or contacts.

All, except during the year 1876, during which the telegraph system had suffered from a hurricane on March 12 and 13, and from storms on November 12 and 13, showed from 1874 to 1875 an increase of faults in the proportion of one to two, whilst the increase in the mileage of line was only in the proportion of 10 to 11, and the increase in the mileage of wire in the proportion of 7 to 8. This disproportion leads naturally to the belief that the financial difficulties had scarcely time to be put straight when they reappeared in consequence of the increase in the number of accidents caused by the aerial lines. It is true that one cannot yet say if underground wires will not, in the long run, give rise to analagous inconveniences.

At the end of the year 1876 the German telegraph administration possessed 450 kilometres of cables, representing 2,941 kilometres of wire. The aerial lines were to the underground lines in the proportion of 30 to 1 for mileage of line, and of 68 to 1 as regards mileage of wire. If we except the underground lines established in the great towns, such as Berlin, Cologne, Frankfort, and Hamburg, which are exposed to accidents by excavation, necessary for the repairs of gas and water pipes, the proportion between the length of aerial lines and underground lines is only as 46 to 1, whilst the relative proportion of faults is as 157 to 1.

This last proportion, although less, is still very favourable to underground lines, even if we take into consideration the fact that the cost of first establishment and also of repairs, is greater for underground than for aerial wires.

However it may be the, line from Berlin to Halle proves the practicability of underground wires, and the working of the lines Halle—Frankfort—Mayence — Strasbourg; Berlin—Hamburg—Kiel—Berlin; Potsdam—Cologne, will prove whether the establishment of underground wires for other than stratagetical or political purposes ought to be preferred to aerial wires.—*Moniteur Industriel Belge.*

IMPERIAL CHINESE TELEGRAPHS.

That such a line of telegraphs really exists and is in daily work is now an established fact, the first line of telegraphs ever erected by the Chinese Government having just been completed by Mr. J. A. Betts, M.S.T.E., Director of the Imperial Torpedo College, Tientsin, and the students of the college. Although the line is but a short one, connecting the offices of the Arsenal with the Yamen of H. E. Li Hung Chang, some six miles distant, its erection marks a new era in the policy of the Chinese authorities. With all the pressure that was put upon the Chinese at Foochow, the Great Northern Telegraph Company failed to get the line through from that port to Amoy, and after two years of hard fighting, the erection of the line was finally abandoned by the company. The line at Tientsin has been put up under very different conditions, being voluntarily erected by the Government with native labour. Not the slightest opposition was offered by the people, permission was readily given to cross the roofs of the houses in Tientsin, or place poles in their courtyards when necessary—not an angry word was heard, and the line was got through with considerably less difficulty than is frequently experienced in England. The River Pei ho is crossed by a piece of submarine cable, with test boxes placed in the terminal poles on each side. The instruments used are Morse inkers, worked by Leclanché batteries. H. E. Li Hung Chang takes great interest in the work, and spends many hours in the office, which is one of the private rooms of his Yamen.

The success of this line at once proves the ability of the Chinese youth to learn the technicality of foreign science, and the absurdity that "Fung Shin" has anything to do with the opposition which telegraphs and railways have hitherto experienced in China. Mr. Betts has been requested to proceed to Formosa with some of his students to construct a line along the Western Coast of the island from Kee Lung to Tai-wan-foo, and a line from Tientsin to the provincial capital, "Paou-ting-foo," is also contemplated.

Notes.

The scientific library and instruments of Mr. F. C. Webb were sold by auction at the Cannon Street Hotel, on Saturday, the 8th inst. There were a number of interesting pamphlets and parliamentary papers relating to telegraphy amongst the books.

Thirteen synchronous electric clocks are being erected by the Corporation of Glasgow on conspicuous points of the city. For some time the disparity of time between the existing town clocks has been matter of complaint. The new clocks will have large white faces and black figures, so as to be plain by day, and readable even by night.

Telegraphy at the Antipodes.—The line in course of construction between South and Western Australia is rapidly approaching completion. The South Australia portion has been built with iron poles.

In New Zealand, during the first three months of this year, 292,308 telegrams were transmitted, being 21,996 more than were transmitted during the corresponding period last year. In cash revenue, these numbers are represented by £16,337, and £670, respectively.

A cable is to be laid direct between Grisselhamn, in Sweden, and Aland, in Russia.

Another articulating telephone, called by the inventor, Mr. G. B. Richmond, an "electro-hydro telephone," has just been patented in the United States. It is similar to Edison's in some respects; but instead of carbon as a varying resistance, water is employed. Two platinum points immersed in water are connected in circuit with the line and battery. One of these points is attached to a thin metallic diaphragm, which vibrates with the sound of the voice. The vibrations of the diaphragm carrying this point to and from the other point, thus diminishing or increasing the electric resistance of the thickness of water between the points, and altering in a corresponding manner the strength of the current in the circuit. The receiver is of the ordinary construction.

We have seen a novel contrivance for enabling an observer to read the scale of a reflecting galvanometer by daylight, which deserves to be better known. It originated, we believe, in the Cavendish Laboratory, at Cambridge. In this plan, the beam of light from the lamp falls in the ordinary manner on the mirror, but instead of being directly reflected back on to the scale, it is diverted by a piece of looking-glass in a direction at right angles, that is to say, in a direction parallel to the plane of the mirror, instead of normal to it. For this purpose the looking-glass is set at an angle of 45° to the plane of the mirror, or face of the galvanometer. The beam so reflected is made to pass through a semi-transparent scale of ground glass or lucent horn, &c., interposed between it and the observer, to whom it appears as a yellow spot of light easily distinguished in our ordinary daylight. It has one defect; the spot is only seen when the observer's line of sight is normal, or nearly so, to the scale. Looked at from one side of the scale, the spot vanishes. But by using a curved reflector this may be overcome.

The fire which recently occurred at the Western Union Telegraph Office, in New York city, was one of those incidental circumstances in the operation of a great enterprise that imparts a lesson of experience. The cause was defective insulation of wires that came in contact in what is known as the "grand switch." This switch is situated in an upper story, and consists of a mahogany table about 25 feet long, and 5 feet broad. It is of elegant cabinet work, placed vertically, and contains about 400 wires, which pass from the battery room through apertures in the ceiling into the switch. It also controls about 10,000 connections. It is, in fact, a systematised combination of all the wires issuing from the chief office to every part of the country. These wires, as they enter the switch, are separated and insulated. By some means two of the wires, not sufficiently insulated, came in contact with each other. Electrical heat was generated, which soon set the wood of the switch on fire. When discovered, the conflagration could not have been continued over ten minutes, yet the time was sufficient to seriously damage the elegant wood work, warp and twist the multitude of wires into every conceivable shape, and render them inoperative and worthless for future use. The actual loss to the building will not exceed 700 dollars, but the fire caused a temporary cessation of the business of the department, which, together with the loss of wires, switch, and the extra labour entailed, it is claimed, will make the loss to the company about 20,000 dollars.—*Scientific American.*

The Gegner prize of the Academy of Sciences, Paris, has been bestowed on M. Gaugain, who is the author of several important researches on pyro-electricity, thermo-electricity, the propagation of electric currents, and the distribution of magnetism.

We have received the "Preliminary Catalogue of the Apparatus in the Telegraph Museum" of the Imperial College of Engineering, Tokei, Japan. The catalogue comprises a fair number of telegraph instruments, specimens of batteries, line wire, posts, insulators, cables, and tools. Professor Ayrton, the writer, has followed the good plan of giving some leading information about the articles exhibited. The following is what he says about the materials used for insulators: "*Glass*, advantages.—Specific resistance, high, smooth surface, so as not to accumulate dirt. Disadvantages—brittle, very hygroscopic, and therefore causing much surface leakage, almost abandoned, except by Mr. Brooks, of America, who is said to have obtained good results with blown glass. *Ebonite*, advantages.—Specific resistance high, surface can be made very smooth, strong. Disadvantages—surface becomes soft and spongy by exposure to the weather, so that dirt collects after a time, causing great surface leakage. *Brown earthenware*, advantages.—Specific resistance moderately high, can be glazed fairly well, and can be manufactured with great uniformity of quality, cheap. Disadvantages—the specific resistance not nearly as high as that of glass, ebonite, and porcelain, and the glaze not nearly as good as that of the latter. *Porcelain*, advantages.—Specific resistance high, can be well glazed. Disadvantages—varies much in quality, much dearer than earthenware, employed, however, in all the best insulators."

Mr. Charles Burton, A.I.C.E., has returned from Paysandu and Concepcion del Uruguay, between which towns he has been inspecting and valuing a telegraph built under a concession of the Platino-Brasilero Company, and not having been accepted is now proposed to be worked by the enterprising saladerista of Paysandu, D. Antonio Santa Maria. The cable to cross the river is already in Paysandu, and the land line which is constructed is in very good order.

The Amoy-Shangai and Bahia-Rio de Janeiro cables are interrupted.

Owing to the depression in trade, the increase in the amount of postal telegraphic business during the fifteen months ended March 31st, 1876, fell considerably short of the increase of previous years. The number of messages dealt with was 26,440,439, being an increase of 960,000 over the number in the preceding fifteen months.

An important paragraph is introduced into the report relative to the training of "military" telegraphists. It is stated that the area of country to be supervised by officers of the Royal Engineers has been enlarged, and 160 Royal Engineers will be employed within a district comprising the whole of the south of England, from the mouth of the Thames to the Land's-end.

On Friday, September 7th, a series of interesting and valuable experiments were made at Saltaire, near Bradford, with the telephones which were exhibited by Mr. Preece at the British Association. Lord Houghton and Professor Barrett, of Dublin, were, we believe, present.

WE hear that the articulating telephone is likely to become very serviceable in mines. The experiments already made with it promise good results. Vocal communication between all the remote underground galleries and the pit-head cannot but be a safeguard against danger, as well as a means of speeding the ordinary work. With electric lamps and telephones the miners could not but be safer than they are at present.

TRAFFIC RECEIPTS.

1877.	Anglo-American Co.	Brazilian Sub. Co.	Cuba Sub. Co.	Direct Spanish Co.	Direct U.S. Co.	Eastern Co.	Eastern Ex. Co.	Gt. Northern Co.	Indo-Euro. Co.	Mediterranean Ex. Co.	Submarine Co.	West Coast America Co.	Western and Brazilian Co.	West India Co.
	£	£	£	£	£	£	£	£	£	£	£	£	£	£
JAN. ...	37,780	11,469	2,311	983	13,530	42,580	24,147	12,663	... ...	... ...	9,668	2,533*b*	10,100	4,058
FEB. ...	32,330	11,606	2,214	784	15,520	35,050	19,645	12,800	... ...	... ...	8,624	2,980*c*	11,026	3,870
MARCH ...	18,880	11,258	3,025	913	8,110	35,491	22,886	16,115	... ...	... ...	9,915	3,128*d*	11,643	5,854
APRIL ...	18,980	11,539	4,706	1,011	6,930	39,861	23,068	16,151	... ...	... ...	11,324	2,362*e*	10,295	7,729
MAY ...	45,420	12,251	4,109	997	17,370	46,309	25,445	18,427	... ...	... ...	11,048	2,170*f*	10,534	7,508
JUNE ...	40,040	9,742	3,295	887	15,260	45,477	24,126	19,663	... ...	... ...	9,960	4,025*g* 2,225*h*	10,522	*3,671
JULY ...	38,790	9,226	*3,900	834	14,820	42,713	22,224	19,187	... ...	... ...	9,943	1,925	7,881	*5,239
AUGUST ...	41,170	9,407	*2,900	804	15,700	40,428	25,371	17,998	... ...	... ...	9,683	... ...	8,610	*4,367

* Estimated. *b*, four weeks ending 13th January. *c*, four weeks ending 13th February. *d*, four weeks ending 10th March. *e*, four weeks ending 7th April. *f*, four weeks ending 5th May. *g*, four weeks ending 2nd June. *h*, four weeks ending 30th June.

City Notes.

Old Broad Street, Sept. 15, 1877.

WE regret to announce another deplorable suicide. The example set by the Direct United States Cable Company has been speedily followed by the Indo-European Company. Some years ago the directors of the Eastern Telegraph Company made a desperate, though ineffectual, effort to bring about amalgamation with the Indo-European. Last March, in referring to this fact, we ventured to congratulate the directors of the Indo-European upon the escape they had had, pointing out that during the year 1876 the Indo-European proportion of traffic to capital nearly reached 20 per cent., while that of the Eastern did not exceed 9 per cent.* We confess we little thought at the time how soon the directors of the Indo-European would forfeit the high opinion we entertained of their sagacity. As a correspondent, whose knowledge of telegraphy entitles his remarks to respect, writes: "It is really surprising how directors, apparently so independent and disinterested, can allow themselves to be talked over by the smooth phrases of a great monopolist, and commit themselves to an agreement such as proposed, before having exhausted every means of competition in order to obtain more favourable terms for their shareholders." Our correspondent goes on to aver that competition has never been honestly tried by the Indo-European Company, "Or else how is it that duplexing has never been tried on that line which is about the most favourable for that purpose?" "If," he pertinently asks, "Schwendler has duplexed stretches of 1,200 miles in India for years, why was he not called in to duplex their sections which are much shorter?" Finally, "Was it in fear they might knock off double the work and abstract too much traffic from the lines of the monopolists? or was it a secret longing at any price for the now cut and dried joint-purse agreement?" We are disposed to think that there has been a secret longing for the joint-purse agreement. If so, it is at length gratified. On the 27th of August a circular was issued by the directors of the Indo-European announcing that a co-operative working agreement for a joint-purse between that Company and the Eastern had been arrived at by the boards of the two companies, subject to the sanction of the concessionary governments interested—(in the *next* paragraph it was stated that the agreement had "already been submitted to the concessionary governments, who had approved of it")—and the approval of the shareholders. On the 11th of September extraordinary general meetings of both companies were held, and the necessary approval of the shareholders of both obtained

It is impossible to undo what has been done, and we suppose the Indo-European and the Eastern Companies must, like the Direct and the Anglo-American, be left to their fate. But though any protest of ours is too late, we do not hesitate to assert that the interests of the shareholders of the Indo-European Company cannot fail to suffer by this ill-advised step the directors have been induced to take. We deny that "the co-operation of the two companies will considerably benefit the traffic," if by traffic is meant that of the Indo-European, We can quite understand that the traffic of the Eastern Company will be increased by the arrangement. We maintain that the amalgamation will have an injurious effect upon the officers of the companies; that it cannot fail to discourage electricians who are seeking to develope telegraphy, and that it will operate disadvantageously as far as the public is concerned. Mr. Pender in fact, intimated at the meeting of Eastern shareholders, that the Eastern Company would now be able to put up its rates, and it may be expected that that will be one of the first things which the united companies will carry out in the interests of the public. We should like to know who are the large shareholders who approved of the agreement before it became public property, and whether Messrs. Siemens were so enamoured with it as the directors of the Indo-European Company wish us to imagine. When Colonel Holland and his colleagues made up their minds that the joint-purse scheme should be put into effect, they might, at least, have compelled the directors of the Eastern Company to agree to better terms. From the agreement one might suppose that the amalgamation was suggested by the shareholders of the Indo-European instead of by Mr. Pender and his friends. Once more, the clique of speculators has achieved a signal triumph on this occasion, too, without the slightest difficulty. They appear to have prepared the terms of amalgamation and to have obtained their enforcement almost at a single stroke. They, we reiterate, have shown their dexterity; but their victims will find the result of the union to be very different from what they anticipate. Another huge job has been perpetrated; unfortunately, retribution will overtake the innocent as well as the guilty.

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* *Vide* "TELEGRAPHIC JOURNAL," No. 98.

It may be hoped that at length the prospect of the unfortunate shareholders of the Western and Brazilian Telegraph Company is beginning to brighten. Judging from some remarks at the close of Mr. Abbott's circular for the current month, we conclude that our advice has been disregarded, and that the Wizard of Tokenhouse Yard has *not* been called in to aid in reorganising the company. The meeting last Tuesday was decidedly stormy, nor did it end very satisfactorily. We think that the interests of the shareholders will be promoted by the election of Messrs. Weaver and Fuller to seats at the Board, but we are not disposed to go so far as Mr. Abbott, who in his condemnation is nothing if not sweeping. It is true that the present Board has done badly enough, but there are useful men on it. Some of the speakers at the meeting alluded to the fact that certain of the existing directors are also directors of the South-Eastern Railway Company, as if they thought that the less the Western Brazilian had to do with any one connected with the South-Eastern Company the better. But the South-Eastern Railway is managed with great ability; and if only the Western Brazilian could be made equally successful, financially, the shareholders would have reason to be pleased. If any attempt should hereafter be hazarded to amalgamate the Western Brazilian with one of the other companies, it should be resisted. The Western Brazilian would possibly gain nothing by the amalgamation, while any other enterprise would undoubtedly lose by the union. We, however, join Mr. Abbott in insisting that it is high time there was peace in the board-room of the company. Until the directors themselves cease to quarrel, the shareholders may rest assured they will not get any dividend. Directors, who, come what may, remain passive, are not worthy of the name; but directors who avail themselves of the opportunity afforded by a meeting to fight each other show that they completely forget the interests they were elected to protect.

In the course of his comments upon the value of telegraph securities, Mr. Abbott naively says: "And now that a friendly working agreement has been entered into with the Direct Cable Company, the shareholders of both undertakings (the Anglo and the Direct) may hope to obtain some reward for their enterprise." But it seems to have occurred to Mr. Abbott immediately afterwards that the reward may not be nigh at hand. So he goes on in the following strain: "But, just now, when the risks and difficulties attending Atlantic telegraphy have been demonstrated by the failure of the expedition to restore the 1865 cable, it is stated that the United States Government purposes taking steps which will directly or indirectly damage the existing Atlantic companies." Naturally, Mr. Abbott places little reliance upon this statement. In truth, investors are assured upon his word that the American Government will not dream of practically confiscating the shore end of the cable of the Direct Company, "because the latter has assumed a position which prevents a destructive competition for traffic, and consequent total loss of dividends on both undertakings." There was a time, not very long ago either, when Mr. Abbott's opinion of the Direct Company was not quite so flattering. But let that pass. The closing words of the paragraph are more amusing, because of their sublime audacity. "There is not," avers Mr. Pender's assistant, "the remotest idea on the part of either company of establishing a monopoly traffic, and this being made clear and secure, the object the United States Government has in view, in their recent action, will have been doubtless attained." Mr. Abbott may not have attached much importance to the words, but everything really turns upon it being "made clear" to the American Government that the companies have no desire to establish "a monopoly traffic." If, in the face of what has happened, they decide that a monopoly traffic is the last thing in the world Mr. Pender and his friends wish to establish, we shall begin to fancy that these gentlemen would succeed in hoodwinking the Prime Minister and the Chancellor of the Exchequer together. Does our recollection play us false? Or is it not a fact that amalgamation between the Anglo and Direct Companies was strenuously urged by Mr. Pender on the ground that competition would thus become impossible? Mr. Abbott ought to let his chief look over the proof of his circular before he returns it to the printer.

One more word touching Mr. Abbott. We have received a pamphlet consisting of a number of reports of various meetings, published by W. J. Johnson, 121, Fleet Street. The first report is that of the Telegraph and Construction and Maintenance Company. It is a curious circumstance that at each of the five meetings Mr. Abbott appears to have been *facile princeps*.

Five telephones are reported to be now in use in New York. One of these connects the office of the Clyde Steamship Line with the wharf from which its vessels sail, and another connects the piers of Brooklyn Bridge with the superintendent's office. The current of sound in these telephones is carried by a single wire in either direction.

Correspondence.

To the Editor of THE TELEGRAPHIC JOURNAL.

SIR,—In Vol. I., No. 1, November 15, 1872, page 13, of your journal, "Deschanel's Natural Philosophy, translated and edited by Dr. Everett, part III., "Electricity and Magnetism," is advertised. On purchasing this book, published in 1872, the following *sixteen* paragraphs appear *non est*:—414, 452, 456, 457, 458, 471, 474, 475, 482, 498, 507, 510, 512, 516, 550, and 594. Will you, or anybody else, kindly tell us if these omissions have been supplied in any subsequent edition? The book has been found exceedingly valuable in our hands, and we should really feel very grateful if we could get a complete edition.

In Vol. I., No. II., December 15, 1872, page 21, pargraph 1, of your journal, under the subject of "Ohm's Law and Kirchhoff's Corollaries, by Robert Sabine," it is stated "the ,following note * * * * is extracted from the proof pages of a *new* edition of the author's work, now in the press."

It is now nearly five years that we have been anxiously looking out for this famous electrician's valuable book, and feel very sadly disappointed at hearing nothing of its issue. As Mr. Robert Sabine's first edition, viz.: "The History, Progress, Science, and Practice of the Electric Telegraph," published in 1867, *ten years ago*, has, we are happy to say, fulfilled its object, by doing hundreds and hundreds of us much good, we sincerely hope that his new book will be *up to date* in *all* scientific matters, and will, *at least*, contain 1,000 pages. Kindly let us know when the book may be expected to be ready.

Our most valuable scientific friend, Mr. W. H. Preece, after launching us in the midst of the vast ocean of the "DUPLEX SYSTEM," has suddenly cut the ropes. The last words, *only three* in number, we have received from his valuable *electric* pen, are to be found in Vol. I., No. XVIII., November 1, 1873, page 311, and they are we regret to say, "TO BE CONTINUED." We have diligently searched in all the other subsequent volumes of the journal for the "continuation" of the subject, but they are nowhere. We have been introduced to his handsome little "*Multum in parvo*," entitled "TELEGRAPHY, by Preece and Sievewright," published in 1876; but although the subject is found there, we could not expect *that* elaborately-written "continuation" of the subject in this little volume, and we beg that Mr. Preece will continue his instructive subject, and lead us to the end of the chapter. Let us also offer our heartfelt thanks to our very valuable learned and electrical friend, Mr. R. S. Culley, for giving us his very valuable and useful "Handbook of Practical Telegraphy," since 1866, and revising it for us *six times*. May we expect a *seventh edition*, *up to date* in *all* scientific matters, before he retires from the Postal Telegraph service, thus leaving a memorable token of his abilities to his friends at home and abroad.

Kindly let us know if the following books can be obtained, and from where:

1. English translation of Count Th. du Moncel's valuable new work, entitled, "Exposé des Applications de l'Electricité," in four volumes.
2. F. C. Webb's "Electric Accumulation and Conduction. Part II."
3. "Electricity," by Esson.
4. "Magnetism," by ——.

} Clarendon Press Series, mentioned in the Catalogue attached to Vol. II. of Clerk Maxwell's "Electricty and Magnetism."

By inserting the replies to the above inquiries in your columns we shall feel extremely obliged.

VOICES FROM THE INDIAN TELEGRAPHS.

[The first part of the new edition of Mr. Sabine's new work, viz., the part bearing on the history and progress of the "Electric Telegraph," was published some time ago as one of "Weale's Series." The second part on the "Theory of the Electric Telegraph" was never published, nor are we aware that it is yet contemplated to be so.

Mr. Preece's papers on the "Duplex System" were discontinued owing to pressure of work. We cannot hold out any hope at present that they will be again continued.

There is as yet no English translation of du Moncel's work published.

The second part of Mr. F. C. Webb's papers on "Electrical Accumulation and Conduction" was never published.—EDIT. TELEGRAPHIC JOURNAL.]

To the Editor of THE TELEGRAPHIC JOURNAL.

SIR,—In the recent number of your journal for August 15, page 193, you express the wish to know what is the correct spelling of the name which you have seen spelt—Menotti, Minotti, Menotto, and Minotto, by various authors.

The true name of the inventor of the pile in question is "Minotto." Professor Giovanni Minotto, of Venice, was Inspector-in-Chief in the Italian Telegraph Administration.

Accept, Sir, the assurance, &c.

D'AMICO.

Director-General of Italian Telegraphs.

Florence, *August 30th*, 1877.

To the Editor of THE TELEGRAPHIC JOURNAL.

SIR,—In your journal, May 15, there was an account of "Recently Adopted Methods of Landing Shore Ends of Cables," as practised by the I. R. G.P. and Telegraph Works Company, in July and August, 1875, and also in December, 1872, by means of empty barrels. I shall be glad to know who claims to be the inventor of that plan, and when and where it was first used, as in the account no name is given.

Yours truly,

R. S. NEWALL.

Ferndene, Gateshead,
September 8, 1877.

To Correspondents.

⁂ Duly authenticated contributions, theoretical and practical, on every subject identified with the interests of which THE TELEGRAPHIC JOURNAL is the organ, will always command attention. Anonymous correspondence will be wholly disregarded. Literary communications and books for review should be addressed to the EDITOR; business communications to the MANAGER, 10, Paternoster Row, E.C.

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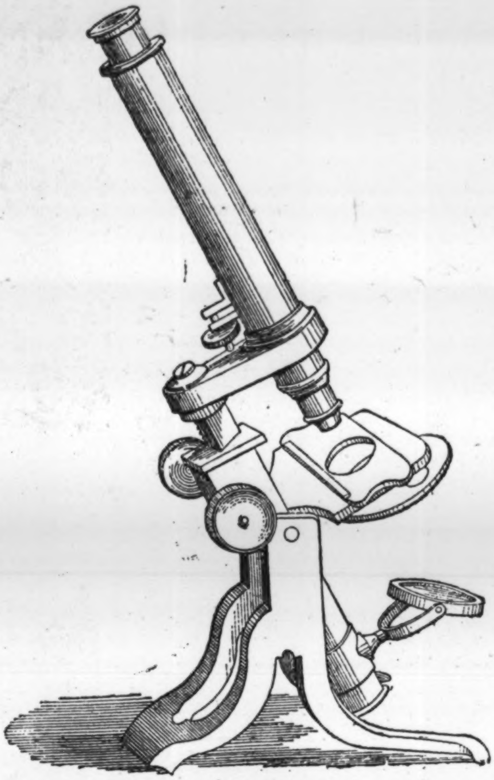

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